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Compliments of Dr Smart

on the Mountain Fever

Malarious waters

Presented

by the Author

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ON

MOUNTAIN FEVER AND MALARIOUS WATERS.

BY

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In the Rocky Mountain region of this country there prevails a fever to which the *vox populi* has attached the name of *mountain fever*. Ignorance of the nature of disease has no doubt fitted this name on many a case to which a little medical knowledge would have applied a more specific title. The writer, for instance, has been called to a case of mountain fever which proved, on examination, to be one of pneumonia. Such mistakes are natural. But, within narrower limits, this looseness of diagnosis is not confined to the non-professional mind. Whether we have an endemic disease in these mountain regions, separate and distinct from the diseased conditions which are recognized by the profession elsewhere, is a question which many a practitioner, who has seen mountain fever, would hesitate before answering. The first object of this paper will be to attempt a settlement of this point.

In the days of the pioneers and prospectors of this western country, mountain fever was more common and deadly than it is now. This is popular testimony, and would weigh but little were it not corroborated by professional observation at later dates. Severe cases of mountain fever were of frequent occurrence among the emigrants who crossed the country on their way to California or the Mormon settlements.[1] And the record of these past attacks appears in practice at the present time. An investigation of previous history, as in life insurance examinations, will often

[1] Dr. Williamson, in the discussion on Mountain Fever. Salt Lake Medical Society, March 12, 1877.

bring forth an account of serious temporary disablement from mountain fever, either on the overland journey or immediately after arrival in the mountain country.[1] In these earlier days the profession treated this fever by mercurialization ;[2] the laity by the "time-honoured sage tea."[3] To whom belongs the credit of having first attacked it with quinia fails to appear upon the record.

But, although neither so general nor so dangerous now as when the emigrants plodded westward with their broken-down stock and overladen wagons, it appears in a severe form with sufficient frequency to make it a matter of much interest to the profession, irrespective of the light which its study seems to throw, as will appear hereafter, on questions of larger sanitary import.

The literature of this fever is exceedingly meagre, consisting only of papers by four medical men and scattered remarks by army medical officers.

In 1851 Dr. C. E. Boyle published an article on "Mountain Fever."[4] In the same year Dr. J. E. Oatman presented the subject under the caption of "Mountain and Malarious Fevers, produced by the same cause."[5] In 1855 Dr. Ewing wrote on "Mountain Fever" in the *St. Louis Medical and Surgical Journal*;[6] and, lastly, in 1865, Dr. F. Rice Waggoner, in the July number of the *American Journal of the Medical Sciences*, published an article entitled, "On the Mountain or Continued Miasmatic Fever of Colorado Territory."

Dr. Waggoner was taught by his pioneer brethren that mountain fever was a modification of typhoid or enteric fever, but he shows clearly in his paper that what he looked upon as mountain fever were cases of purely malarial origin—severe remittents, unremitting remittents, so to speak, or, as he calls them, continued miasmatic fevers.

Fort Lyon, Colorado, at which Dr. Waggoner treated his cases, was situated in the valley of the Arkansas River, with thousands of marshy acres around it, with a summer temperature running as high as 105° F., with heavy autumnal rains, and a sick report bristling with intermittent and remittent fevers. Two years after his article was published the post was abandoned on account of its unhealthy location, and a new site having been selected on higher ground, about twenty miles distant from the old one, the garrison was relieved from its liability to this fever.

Dr. Waggoner's paper was written not so much to prove the malarial origin of the mountain fever of Colorado as he observed it, as to insist on

[1] Dr. Benedict, in the aforesaid discussion. Salt Lake City Medical Society, March 12, 1877.

[2] Dr. Allen Fowler. [3] Dr. Benedict.

[4] Ohio Medical and Surgical Journal, vol. iii. pp. 528–530; and St. Louis Medical Journal, vol. ix. pp. 454–456.

[5] North Western Medical and Surgical Journal, vol. viii. pp. 105–108; and Boston Medical and Surgical Journal, vol. xliv. pp. 511–512.

[6] Vol. xiii. pp. 109–116.

large doses of quinia in its treatment—sixty to seventy-five grains in divided doses during the twenty-four hours. This treatment, however, has been practised by army medical officers in the fever districts of the Gulf coast, the southern States, and western territories since the days of the Florida war.[1]

But, if Dr. Waggoner found his cases to be so clearly malarial, those of Dr. J. E. Oatman were apparently met with under different circumstances, if we may judge from the title of his article—"Mountain and Malarious Fevers, produced by the same cause." There must have been some doubt as to the cause to call forth the argument. I regret that I have been unable to lay hands on the articles written by Drs. Oatman, Ewing, and Boyle.

To come to more recent dates, I find in the Report on the Hygiene of the United States Army, published in 1875, that various medical officers take cognizance of mountain fever.

Surgeon E. P. Vollum, U. S. A., my predecessor at this post, Camp Douglas, Utah, reports[2] that: "About once in ten years an epidemic of *mountain fever* appears to a considerable extent throughout the Rocky Mountain regions including Utah. Its last appearance was in the fall and winter of 1871–72. It is a malarial fever, commencing as an intermittent, passing on to a remittent, then into a typhoid condition. It may often be cut short by prompt large doses of quinia, but after the typhoid symptoms set in it should be regarded as typhoid fever and so treated. The mortality is often high, but reduced in proportion to the attention a patient receives in the early stages."

The register of the undertaker of Salt Lake City, as quoted by Dr. Vollum himself,[3] does not bear out his statement as to its occasional epidemic virulence, for the percentage of deaths from fever to deaths from all causes—

for 1871 was 11.33
" 1872 " 10.24
" 1873 " 13.20
" 1874 " 12.46

showing, at least, that if mountain fever visited the city in 1871–72 to a greater extent than in the two following years, it failed to leave its mark on the undertaker's books.

Dr. A. J. Hogg, from Medicine Bow, Wyoming Territory, reports[4] that: "Occasionally there is a case of a fever vaguely called mountain fever, but which resembles the remittent type more than any other, and is very amenable to treatment."

[1] For many reports on this head see Medical Statistics U. S. Army, 1839–55, pp. 637–690.

[2] Page 340. Report on Hygiene of the U. S. Army, 1875.

[3] Page 341. Ibid.

[4] Page 361. Ibid.

Assistant Surgeon Patzki, at Fort Steele, Wyoming Territory, reports[1] that—

"A remittent fever, occasionally very severe, is met with, by the mountaineers called mountain fever, and much dreaded by them. The most prominent symptoms are headache, severe aching through the whole body, insomnia, furred tongue, frequent, full pulse, constipation. Chills are infrequent. The efficacy of large doses of quinine proves the malarial origin. The mountaineers treat it with their panacea, sage tea, and, as they assert, quite successfully. Men cutting timber along the streams, mostly Danes and Swedes, suffer most from this fever."

Surgeon F. L. Town, U. S. A., at Fort Shaw, Montana, states[2] that:—

"Remittent and typho-malarial, and, probably, enteric fevers, are not infrequent in the spring and fall, especially among miners and hunters, or persons who are generally without shelter; these, in the parlance of the country, are called 'mountain fevers' indiscriminately. Three cases of typho-malarial fever have occurred at the post, two of citizens and one a soldier, and with a fatal termination in each instance."

Lastly, Assistant Surgeon Jaquette, U. S. A., at Fort Boise, Idaho, reports[3] "an occasional case of fever, either remittent or intermittent, commonly called in this country mountain or typho-malarial fever."

Dr. Williamson, of Salt Lake City,[4] while admitting the resemblance to malarial remittent, and the efficacy of quinia, when administered early in the disease, feels confident, from an extended experience, that if the fever has fairly established itself before the patient is seen, "you will have a case of disease that will run from three to five weeks before you can see the commencement of convalescence."

Dr. Allen Fowler's experience[5] has led him to abandon the name of "mountain fever." The disease to which he formerly applied that title he now recognizes as malarial remittent fever.

From the above extracts it will be seen that the published testimony indicates the mountain fever of popular parlance as consisting of malarial remittents with a possibility of typho-malarial or typhoid fevers—that is to say, that we have no specific fever indigenous to the Rocky Mountain regions.

But before discussing this conclusion I desire to place on the record the result of my own observations during a service of nearly four years in the Rocky Mountains.

I arrived at the post of Fort Bridger, Wyoming Territory, in August, 1873, and as the summer was far advanced, expected to come into immediate contact with the mountain fever or malarial remittent of which I had heard much and read but little. Yet there seemed nothing malarious in the surroundings of the post. The drainage was good, the valley fall being so marked as to prevent all stagnation of surface or subsoil water; the vegetation scanty, consisting of sage brush and interspersed grass tufts on the

[1] Page 385. Report on Hygiene of U. S. Army, 1875.

[2] Page 434. Ibid.

[3] Page 460. Ibid.

[4] Discussion on Mountain Fever. Salt Lake City Medical Society, March 12, 1877.

[5] Ibid.

mesas, and a thin belt of shrubby growth along the creek; while the thermometer fell to 32° F. ten months out of the twelve, and the winds blowing most of the time from the northwest, came from the higher peaks of the Wasatch range.

The autumn and winter came and went without the advent of any fever cases either among the troops or the settlers in the vicinity, and I began to consider mountain fever a myth. But as spring advanced a few anomalous cases presented themselves at surgeon's call. The men complained that they had caught cold, and investigation showed the presence of a slight catarrh or of some equally slight congestion of the fauces; a local affection of too trivial a nature to account for the peculiar coexisting constitutional disturbance. The bowels were constipated, urine scanty, skin dry, complexion sallow, pulse about a hundred, and temperature slightly increased, with loss of appetite and nausea, much languor and depression of spirits, pains in the bones and muscles, and stiffness in the joints. Occasionally instead of constipation there would be a marked looseness of the bowels. Not that there was anything peculiar in the symptoms as above enumerated, they simply indicated the febrile condition; but the languor, the muscular pains and articular stiffness were more prominent in the appearance and complaints of the men than the condition of the pulse would have led one to expect. Some thought they were going to be laid up with rheumatism. Many had been ailing for a week or two before they reported as sick, the first signs of deteriorated condition being loss of appetite, disturbed and dreamful sleep, and morning stiffness in the joints, chiefly the metacarpophalangeal and phalangeal. But if anything could be called characteristic of this condition it was the appearance of the tongue, smooth and coated with a thin film like the faint bluish-white advance of the skin over a healthy ulcer in its progress to cicatrization; this, with or without a yellow fur toward the base. As I had often seen this tongue connected with malarial poisoning (it was pointed out to me by Dr. R. C. Stiles, then Surgeon U. S. Vols.), and as in the more aggravated of these cases remissions and exacerbations of the febrile state were readily discoverable, I treated them from the first with quinia, with or without cathartics according as the film on the tongue was clean or yellow-furred, and returned them to duty in a few days.

I felt satisfied that the febrile action was totally unconnected with the slight local congestion—that the latter was in fact an accidental circumstance engrafted by simple exposure on a depraved constitution, and engrafted the more readily on account of that depraved condition.

The cause of this deterioration in the health of the command was difficult to unearth. The men were cleanly, well fed, well housed and clothed, and had just enough of fatigue duty to keep them in healthy condition. Nor did the sutler's store seem to be implicated in the matter, for men well known to be temperate were affected as frequently as those whose

reputation in this respect was not so satisfactory. The appearance of the tongue, the tendency to remission and the influence of quinia seemed to indicate malarial exhalations as the *materies morbi*, and yet in view of the want of malarious surroundings, in view of the season of the year and the temperature which discountenanced a malarial theory, and especially in view of the fact that certain recruits who had arrived at the post prostrated every week or two with intermittent fever were progressing to perfect freedom from aguish attacks, I could not bring myself to record these cases as malarial remittents, but put them down as catarrhs and quinsies, and awaited developments.

These came in a very short time. Cases appeared of fever without any complication by local lesion, and I recognized that I must be dealing with the incipiency of our so-called mountain fever.

At the same time many officers and men in the garrison complained of feeling out of sorts without reporting sick. They had lost appetite, felt stupid and sleepy, had pains in the limbs and stiffness in the joints, and the tongue was covered with the smooth bluish-white film. To these blue pill and quinia, or the latter alone, were given, and no more was heard of them.

The citizens in the neighborhood furnished my first severe case of this fever. A woman residing near the post, who had been in bed for ten days under the sage-tea treatment before I was called, seemed beyond the power of medicine. She was in a typhoid condition, tongue dry and black, great prostration and emaciation, low delirium, frequent stools of bloody mucus, followed in a few hours after I first saw her by a state approaching coma-vigil. Two weeks later she was convalescing after a course of quinia, astringents, brandy and nourishment; but it was long before she recovered her strength.

Next a surveying party in the neighbourhood of Bridger put into the post on account of the sickness of two of its members. One had given up work four or five days, the other two days before their arrival; but both had been feeling out of sorts for a long time before they became really prostrated by their sickness. The latter, the milder or more recent case, presented the symptoms which I have indicated as affecting the enlisted men of the garrison, and like them, under quinia, the patient recovered in a few days: the former differed by having not so much an exaggeration of the febrile state, as in having more languor and depression of spirits, with a tongue in which the film had increased in thickness to form a whitish layer, no yellowish fur being present either at the centre or base. The case ran a typhoid course of four weeks and was similar in its progress to that of the woman already mentioned, but as in her case it was not enteric fever. The exacerbations and remissions were less distinct and more irregular, the temperature was never so high as in typhoid, the diarrhœa was dysenteric in character, and there was no tenderness in the ileo-

cæcal region, while for the amount of febrile action as measured by the pulse and temperature the nervous depression was extreme.

While attending this case I had an opportunity of comparing it with one of pure typhoid occurring in the person of a girl fifteen years old. This case was carefully studied, as it was interesting not only in connection with the mountain fever case, but as bearing upon the origin *de novo* of typhoid fever. At its inception it was viewed as another specimen of the mountain fever, but as it was uninfluenced by quinia, the remedy when pushed producing much gastric disturbance and bilious vomiting, the diagnosis was altered to typhoid, doubtingly, until the eruption established its accuracy. There were an accompanying bronchitis, evening exacerbations, rose-coloured eruption, ileo-cæcal tenderness, and typhoid diarrhœa, all of which were absent in the mountain fever case.

To complete the fever history of that season and fill the niche between the incipient cases in the garrison and the typhoid examples among the citizens, a case occurred in the person of a soldier who was on detail as post-gardener. Soldiers, when indisposed, appear at surgeon's call for excuse from duty when, if in civil life, they would not think of sending for medical advice. Most of the cases in garrison are thus seen at the first manifestations of the disease; but this gardener, being his own master and requiring no excuse from duty, thinking that he would get well in a day or two, failed to report as sick until the fever was well advanced. His tongue on admission into hospital was white-coated like that of the surveyor, but in a week he was convalescing and in two weeks was returned to duty—thus avoiding the slow convalescence consequent on the development of the typhoid stage, yet in his case the tongue became dry and its white coating browned and fissured before the change for the better was inaugurated.

Such having been my experience up to the autumn of 1874, I wrote for the *Report on Hygiene* published in 1875, that at Fort Bridger, "a remittent fever susceptible to the action of quinine is well recognized as being indigenous."

While at this same post during the spring and early summer of 1875, and again in the corresponding season of 1876, a recurrence of this febrile tendency appeared among the troops; but most of the cases were seen early, and in a few only did typhoid symptoms begin to make their appearance.

This closes my experience at that station.

One point, however, remains to be mentioned. It is that the records of the post, the figures left behind them by my predecessors as embodying their experience of the prevalence of mountain fever, corroborate my own observations, showing the months of May, June, and July to be the months of visitation. Thus, during the eight years previous to my assignment, May gave an average of 5.60 cases per thousand of mean strength, June 13.89, and July 9.47; while no cases were recorded during the

months of September and October. Yet Dr. Drake reports those very months of September and October as the period of maximum prevalence of malarial fevers in the United States.[1]

The following table shows the average strength of the command and the prevalence of this fever in cases per thousand of mean strength:—

Month.	Strength.	Cases.
January	172	4.36
February	167	.75
March	168	1.49
April	154	1.62
May	134	5.60
June	135	13.89
July	132	9.47
August	145	4.26
September	131	....
October	156	....
November	171	5.85
December	168	.74
Annual	153	48.03

In July of last year (1876) I reported for duty at Camp Douglas, Salt Lake City, Utah, but saw no febrile cases until quite recently. Two companies of the Fourteenth U. S. Infantry, which had been out with General Crook against the hostile Sioux from November, 1876, until the following January, arrived at the post to take station immediately after the campaign was finished. A day or two after their arrival, the men began to report at surgeon's call in a condition similar to that with which I had become familiar at Fort Bridger. A dozen were furnished with a cathartic, and quinia in five grain doses thrice daily, but were not taken on sick report. Five men and one officer, however, had become so oppressed with the mountain fever poison that they had to be reported sick and retained under treatment from three to nineteen days. And it is interesting to remark, as being the only case out of many, that one man, after having been discharged from hospital as entirely recovered, made his appearance in eight days with a recurrence of the febrile attack;[2] but it was readily removed by quinia, and no subsequent relapse has taken place. These men had suffered much from exposure while on their campaign—during the latter part of it especially. At and after Christmas, as I am told, the thermometer was more often below zero than above it.

Such has been my experience of the only peculiar febrile condition which I have observed in a four years' service in the Rocky Mountain country, showing itself in:—

[1] Daniel Drake. The Principal Diseases of the Interior Valley of N. America. Phila. 1854.

[2] I have since met with two similar cases of recurrence. Oct. 20, 1877.

1st. A primary stage of one, two, or more weeks, during which the individual is more or less oppressed by the influence of the *materies morbi*.

2d. The development of fever, more or less marked, and more or less rapid in its course, with irregular remissions and much more mental depression and muscular prostration than the patient's pulse and temperature would prepare the observer to find.

3d. A typhoid stage marked by prostration, emaciation, low delirium and coma-vigil.

It is to be observed that I have seen but few cases in the typhoid stage, and few showing the transition to it from the remittent condition, but, nevertheless, I do not hesitate in my opinion that these three conditions are produced by the action of one and the same morbid influence, and that they constitute the infancy, adolescence, and maturity of the mountain fever of the pioneers. And in this opinion I have most important support —for a summary of the above description of mountain fever read before the Salt Lake City Medical Society, March 12, 1877, received the sanction of that body.

Now, the question comes, Is this a fever *sui generis*, or are we to give it recognition as typhoid, typho-malarial, or malarial remittent fever?

And first as to typhoid. As already seen, the preponderance of the published testimony is not in favour of this theory. Yet, beside the generic traits of fever certain points of similarity exist between this disease and enteric fever; for instance, the languor, depression of spirits, and intellectual oppression, the remissions and exacerbations, the typhoid tongue, prostration, and delirium. But closer investigation shows many and marked differences. It runs no definite course; for in one case the patient may have been out of sorts for weeks before the febrile action is developed, while in another the first stage may comprise only a few days. In one the fever may go on from day to day (in the absence of proper treatment) without much apparent change for the worse, while in another a day or two may suffice to manifest the desiccation and darkening of the tongue. This same irregularity applies to the history of the individual case as regards the occurrence of the remissions and exacerbations. The temperature does not run so high as in typhoid. No eruption is presented, no meteorism, no iliac tenderness; and if diarrhœa be present, as it usually is, it is dysenteric in character. Again, easily recognizable cases of typhoid fever are rare in the Rocky Mountains, especially in the remote settlements, while mountain fever is so common, especially in those same settlements, as to be a name in the mouth of the people. At Fort Bridger, for instance, out of a mean strength of 153 men, there were recorded during the eight years, 1866–73, 59 cases of mountain fever expressed as malarial remittent, and but one of enteric fever. Lastly, quinia has no power to check the onward course of typhoid, while its influence over this fever, particularly in its first and second stage, is most marked.

As to the typho-malarial suggestions, if that fever be viewed as a com-

posite, typhoid occurring in a constitution broken down by exposure to malaria, it is excluded by the exclusion of typhoid. If, on the other hand, it be considered as a peculiar manifestation of malarial poisoning, the suggestion remains to be discussed along with the malarial remittent theory of mountain fever.

Fever from malarial poisoning is so many-faced in its appearance that it cannot be excluded so readily as the typhoid by simple observation of the symptoms. In fact, at first sight one is struck by resemblances rather than by differences, as witness the similarity of the first stage to the dumb ague of malarious districts, the remissions of the second stage, and the influence of quinia.

With regard to the effect of quinia, there appears to be a tendency in the profession to bring in malaria guilty if this remedy testifies against it, although the specification may be unsustained by any other witness. It is poor logic when put down in black and white:—

Quinia is a specific in malarial disease.
Quinia is a specific in x.
x is therefore a malarial disease.

But it is a very good working rule, expressing as it does the greater likelihood that the source of the malarial poison has escaped our observation, than that quinia, in view of our long experience of it, should be a specific in other genera of disease. Dr. Patzki, of Fort Steele, illustrates this tendency by saying, as quoted above: "The efficacy of large doses of quinia proves its malarial origin," while he immediately continues:[1]—

"That persons afflicted with ague rapidly recover in this climate was illustrated in June and July, 1867, when the troops brought from the swamps of Florida had their systems tainted with this disease. During the two months 96 were rendered unfit for duty out of a mean strength of about 200 men, and many more suffered to a less degree. During the next four months but 17 cases occurred, and none during the winter."

On page 319 of the *Report on Hygiene* I find myself reporting from Fort Bridger that:—

"The intermittents are imported diseases. During my service at this station, I have found no case which originated in the locality. On the contrary, the tendency in the imported cases is to longer intervals and ultimate recovery. Every monthly report which shows an unusual number of cases of this disease shows at the same time some change in the garrison. During succeedings months the number becomes smaller, until a new company or a detachment of recruits brings a fresh influx of intermittent cases. In one notable instance, occurring in June, 1869, when the garrison was relieved by troops from Florida, 40 intermittents were taken on sick report in a strength of 199 men, or 200 per thousand for the month, while the average for the year (computed from the eight years, 1866–73) is only 167 per thousand."

The post surgeon of Fort Shaw, Montana, reports on page 431: "I have known of no cases of intermittent fever that have with certainty originated in the country."

Again, if note be taken of the exposures of the two companies of the

[1] Report on the Hygiene of the U. S. Army, page 385.

Fourteenth Infantry, which furnished me with my recent cases, it will be found that they received the mountain fever poison while the temperature was at times below zero, and always below the freezing point—a fact which certainly does not fall in with our established notions of malarial poisoning.

If then mountain fever is a malarial remittent, as the general testimony seems to determine it, the question arises—How can we have a malarial fever in a region of country where there are seemingly no malarious traits, and where it is noted that sufferers from malarial poisoning rapidly throw off the thrall of the disease?

Do we have other telluric influences—other than the marsh malaria—a mountain miasm for instance, similar to, but differing in its habits from the swamp poison, and producing a disease generically similar to but specifically distinct from our malarial fevers?

Or, can we have malaria, exhaled from the lowland swamps, transported, to Fort Bridger for instance, across vast tracts of interlying country, and this in the face of the steady northwest wind which blows over the post from the higher regions of the mountain range, the said malaria being so modified in its transit as to produce the mountain remittent with its peculiarity of symptoms at seasons which are not the seasons of visitation in recognized malarious districts?

In this connection I desire to refer to certain experiments which seem to throw light on the causation of this fever.

While stationed at Fort Bridger I spent several months in examining the various spring, well, and river waters made use of by the troops in Nebraska, Wyoming, and Utah. The most remarkable point developed by these analyses was the large amount of undecomposed organic matter of vegetable origin which was contained by all the river waters, even those which were looked upon as pure mountain streams. That the organic matter was vegetable in character was inferred from the absence of the sodium chloride which is the invariable accompaniment of animal matter; that it was recent, or at least in good preservation, by the absence of the nitrites which would have resulted from its decomposition.

For the determination of this organic matter Wanklyn and Chapman's process was adopted, as affording more delicate results than the coarse method by ignition, and more trustworthy than the permanganate decolorations. This process depends on the transformation of the nitrogen of the organic matter into ammonia by distillation with permanganate and caustic potash, and the estimation of the resulting ammonia by Nessler's solution. The facts developed have such an important bearing on the question of water-supply that I shall detail the steps of the process, that I may guarantee the results to those who have a practical knowledge of its manipulations.

500 cubic centimetres of the given water were distilled from a large retort, connected with a Liebig's condenser, until 50 c. c. had collected in the receiver. This distillate contained most of the free ammonia of the

water. It was estimated by means of Nessler's solution. 50 c. c. more were then distilled and the ammonia determined as before. If the second measure of 50 c. c. showed the presence of ammonia a third was distilled, and, if necessary, a fourth, until proof was obtained of its complete removal from the water remaining in the retort. Ten grammes of caustic potash and .400 grm. of permanganate were then dropped into the retort—either dry or in solution in distilled water which had been proved to yield no ammonia when distilled with these chemicals—the distillation continued, and the ammonia evolved in the destruction of the organic matter estimated in successive measures of 50 c. c. of the distillate, until no more was found to be given off.

In these determinations the greatest care was taken in conducting the distillation, in watching the coloration produced by the Nessler's solution, and in proving the strength of the standard ammonia solution.

Now, to appreciate the results which were obtained it must be premised that the authorities in sanitary science, who have had most experience of this method of organic determination in water analysis, lay it down as a rule that water containing .10 part per million of ammonia from nitrogenous matter should be regarded with suspicion, while that which contains .15 ought to be condemned as dangerous to health.

The *purest* of the river waters examined were those of Lodge-Pole Creek at Sidney Barracks, Nebraska, Black's Fork at Fort Bridger, Wyoming, and the Douglas Brook at Camp Douglas, Utah—and these gave respectively .19, .20 and .28 part per million of ammonia from the nitrogen of dissolved organic matter.

This was so singular and so unexpected that I proceeded to investigate the matter more closely. At first I inclined to the opinion that the water must collect this large quantity of organic contamination in its course along the valleys. I thought of the immense amount of decaying vegetation in this wild region where no crops are harvested, but where the growth of to-day uprises from the decay of ages. I gave all credit to the effect of beaver dams in stagnating the waters above them, which afterward found their way slowly into the main current and polluted it with the organic debris which they had dissolved during their stagnation. But this theory was scarcely tenable in the face of the fact that streams running in a rocky bed and with but little vegetation near their radicles were found to be nearly as much impregnated as those which had a slower course in the tangled brush-wood of lower-lying valleys. However, I could form no better theory, and as a step in the direction of testing it, I set to work in the first place to prove the purity of the water which fed these streams.

The springs I had already on my record as pure—Camp Douglas Spring, for instance, containing but .10 part per million; there remained therefore as feeders the rain-fall and the melting of the snow.

The first heavy snow-fall of 1875 occurred about this time; I collected it carefully in clean vessels, melted the samples, and on examination was

surprised to find that it contained nearly twice as much organic matter as the average of the river-waters examined. A second fall two days later gave a like result. The subsequent snow storms of that season yielded less of organic impregnation, although in all cases it was in excess of that contained by the waters of the running streams.

At this time I did not connect the vegetable impurity in the snow-water with any practical question, except in so far as to conclude that the organic matter with which we had to do in our western streams must differ in quality from that which British health officers found in *their* water analyses, and that instead of being guided by *their* rules in forming our opinion as to the wholesomeness of a water we must form rules based upon our own experience. And as Black's Fork, Douglas Brook, and Lodge-pole Creek were looked upon as pure mountain streams, although their waters contained up to .28 part per million; and again, as I was informed by the medical officer on duty at Fort Sanders, the water supply of which contained .50 part, that certain low fevers which had prevailed in the neighbourhood were vaguely rumored as being connected with impurity in the water—I felt warranted in summarizing that with us water containing less than .30 might be viewed as wholesome, from .30 to .40 suspicious, and from .40 to .50 dangerous.

I was satisfied with having detected the origin of the organic taint in our so-called pure streams: that it consisted of vegetable emanations and debris swept up by the winds from the face of the continent and precipitated by cold and moisture along with the snow from the higher regions of the atmosphere.

But, that it was productive of injurious effects on the human system, except when existing in unusual and excessive proportion, and that I had seen and treated these injurious effects and speculated on their hidden cause, did not occur to me until the following spring, when I found myself again face to face with the mountain remittent, and saw the stream of Black's Fork, which furnished the water supply of the post, swollen to thrice its usual volume by the melting snows, and charged, as I supposed must be the case, with the larger amount of organic matter—which the predominance of snow-water gave to the stream. Then it was that I referred to my laboratory note-book for various determinations of the organic matter in Black's Fork, which I had made while puzzled as to whence the contamination was derived; and there I found recorded—

Black's Fork water	June 14, 1875,	.28	part.
" "	July 19, "	.24	"
" "	Aug. 28, "	.20	"
" "	Oct. 12, "	.16	"

Another recorded experiment is interesting as showing the organic contamination of this stream when at its purest. It was performed simply to ascertain the organic impurity in the ice-supply of the post, but as this ice

was the solidification of the water during the winter months when free from all contamination by melting snows, the result was now viewed as expressly the minimum impurity of Black's Fork.

Melted ice from ice-house, stored in February, 1875, issued for use and examined August 14, 1875, contained .14 part per million.

The testimony of these figures in favor of a water origin of the fever, showing a maximum and minimum of organic taint corresponding with a maximum and minimum of visitation, was so direct that I felt chagrined at having failed to recognize it sooner—that my search for the origin of the impurity had so preoccupied me as to exclude from the mental field that view of the results of the contamination which now seemed to have lain so plainly exposed.

To complete the series two examinations in 1876 may be given :—

Black's Fork water, April 21, 1876, .20 part.
" " " May 15, " .28 "

Rhetorically we make use of snow as a symbol of purity. Dr. Parkes, however, informs us[1] that "there has long been an opinion that snow-water is unwholesome, but this is based on no reliable observations." The above experiments demonstrate cause sufficient to account for its unwholesomeness, and as to the reliability of the observations, I can only say that they were carefully performed, and that the experience gained in conducting over fifty distillations for organic matter in potable waters, superintended their performance.

Besides the Fort Bridger experiments already referred to, and some half dozen examinations which were not recorded, as giving at the time no new light on the subject, but simply corroborating the accuracy of those first made, I have conducted a series of examinations during the past autumn and winter at Camp Douglas, Utah. They are given below :—

	Free Ammonia.	Organic Ammonia.
1. Snow; large, heavy flakes, November 15, 1875 .	.30	.50
2. Snow; large, heavy flakes, November 17, 1875 .	.30	.50
3. Snow; large, heavy flakes, March 21, 1876 .	.10	.60
4. Snow; small, granulated, October 30, 1876 .	.32	.20
5. Snow; small, granulated, October 30, 1876 .	.32	.22
6. Snow; flakes, December, 22, 1876 . . .	.02	.40
7. Snow; flakes, January 29, 1877 . . .	.04	.46
8. Snow; small flakes, February 3, 1877 . .	.18	.34
9. Snow; large, heavy flakes, March 6, 1877 .	.30	.58
10. Snow; fine, granulated, March 8, 1877 . . .	.30	.22
11. Sleet; March 31, 1877	.28	.28
12. Rain; October 17, 1876	.20	.16
13. Rain; March 29, 1877	.28	.18
14. Rain and sleet; April 2, 1877	.28	.22

[1] Manual of Practical Hygiene. London, 1866, p. 9.

These determinations of the organic matter in snow-water, when taken in connection with determinations by the same process of organic matter in the water of our running streams, amount almost to a demonstration of the origin of the latter. It will be seen that the snow-water contains from .20 to .60 part per million of ammonia from nitrogenous impurity, but the average snow-fall of the year has an impurity approaching more to the higher than the lower figure, since the great mass of the snow which falls on this mountain country, and lies until melted by the warmth of the succeeding spring and summer, contains from .50 to .60 part per million. These are the heavy snow-falls, consisting of large, moist flakes which, continuing for two or three days at a time, pile up so many feet of snow all over the face of the country. The snows which are, comparatively speaking, slightly impregnated with organic matter, are the light granulations which fall in but insignificant showers, and the drifting storms of small, dry crystals, which seldom add more than an inch or two to the winter's covering. Estimating by experience the relation between the heavy snow-falls, with their large proportion of organic matter, and the lighter falls containing a smaller proportion, I feel confident that I am not exceeding the truth in placing the average contamination of snow-water at .45 part per million.

Assuming this figure to be correct, and estimating the organic matter of our streams when purest at .14, the occurrence in Black's Fork during the months of April, May, June, July, and August of .20, .28, .28, .24, .20 seems to be accounted for; the conclusion arrived at being that the impurity of our river water is derived from the atmosphere chiefly through the winter snows. Rain-water is purer in this respect. The degree of cold, no doubt, accounts in a measure for the greater precipitation of organic matter with the snow-flake, but the feathery structure of its crystals, presenting a larger surface for the condensation and entanglement of all floating particles, makes the heavy snow-storm a more efficient purifier of the atmosphere than the rain shower, by sweeping to the earth more of the vegetable debris which may have accumulated in the upper strata of the air.

Of this vegetable debris what is so likely a constituent as the cause of the malaria which emanates from such vast districts of country? Here the question is begged, but under the circumstances is this too much to require? A non-malarious country is affected at a certain season with a malarious disease; this season corresponds with the contamination of the drinking-water by vegetable matter brought from distant regions. Swamp malaria is known to be transported by winds. It is known to rise mist-like and be wafted mountainwards from the valleys in which it is exhaled. And if more ponderable matter of vegetable origin be carried into the higher strata of the atmosphere to be subsequently swept down by the snow-fall, why may not malaria accompany?

Many experiments are recorded to prove the germ theory of disease—that the air around us is charged with invisible molecules potent for evil, and, to my mind, every operation performed after the antiseptic teachings of Mr. Lister is a proof of the existence of such germs. Were these germs continually accumulating without provision for their removal, wherein would consist the value of ventilation? But nature's processes preserve the air we breathe in comparatively pure condition. The snow and rainfall clear the atmosphere of such contaminations. What becomes of the exhaled malaria if it be not swept down in like manner?

Surgeon John S. Billings, U. S. A., in a recent lecture before the medical profession of Baltimore, Md., informs us[1] that—

"The second object in hospital management is the removal of all dust which has settled or lodged, and that this shall be a real removal, and not a mere scattering of it from one place to allow it to settle elsewhere. If, for instance, dust is removed with a damp cloth, this damp cloth becomes a dangerous thing in itself. If the external air be cold we may have a precipitation of moisture on the glass of the windows, and in this moisture will be a considerable proportion of organic matter, so much that if it be collected the fluid will give decided signs of putrefaction. Now this precipitation of moisture and organic matter is temporarily a purifying process."

Yes. And nature's grand precipitation of moisture and organic matter in the form of our winter snow-falls, is also a purifying process, enabling ourselves, in our small way, to purify the atmosphere of our wards by admitting the air which *she* has purified to drive out and dilute that which we have not as yet found means of purifying after *her* fashion by precipitation.

This on the supposition that malaria is an entity separate and distinct from the organic matter which can be chemically recognized, but who shall say that it is so, or that it resides in such and such impalpable particles of the organic matter and not in others? My own opinion is that malaria in the upper regions of the air bears the same relation to the organic matter existing there, and correspondingly when both are precipitated into our water supply, that organic matter in the wards of a hospital bears to the carbonic acid accumulated with it. Carbonic acid, when in large excess, is pernicious from its own peculiar properties, so, no doubt, with the vegetable impurity of the water; but under the ordinary circumstances of our present ventilation system, carbonic acid is insignificant in itself, and becomes of consideration only as a measurer of the otherwise unmeasurable organic matter of our wards. If we have a ward or sleeping room which gives a large proportion of carbonic acid, we infer a correspondingly large proportion of deleterious animal exhalations, and ventilate accordingly. So it may be conceived that a large proportion of vegetable snow-derived organic matter in our potable waters is of consideration mainly as a measurer of the otherwise unmeasurable malaria which it contains.

[1] On the Plans for the Johns Hopkins Hospital at Baltimore. New York Medical Record, March 3, 1877.

This malaria, evolved from the swamps and jungles during the heats of summer and early autumn, is swept off by winds, and would accumulate to a pest cloud enveloping the earth but for the autumnal rains and winter snows which bear it back to the surface of the soil, and bury it ultimately in the ocean; the rivers thus becoming Nature's drains to carry off the sewage of the atmosphere.

Autumnal rains are here mentioned as purifiers, for, although rain-water is purer in respect of recognizable organic matter than that derived from the snow, it by no means follows that it is less charged with disease germs —less charged with malaria from the atmosphere. The test for the presence of malaria in the rain showers falling upon our mountains must be the coexistence of a rain-caused rise in the streams, with an increase of such cases as we have already referred to malaria in the snow. The cases which occur at Fort Bridger in November (see above) show the morbific agent which causes the remittent to have been at work; and it is just at this time that the stream is carrying off the autumn rains which fall upon the mountains. Toward the end of September rain storms begin, and although the fall at Fort Bridger is small, it must be remembered that it is not the fall at the post, but the much greater fall along the mountain ridge which has to be considered.

We may not suppose that all snow is thus charged with malaria and other deleterious matter of vegetable origin. The evaporation from the ocean precipitated as snow upon some island or sea coast surface may be perfectly pure so far as organic impregnation is concerned. The snow which falls on the Polar Seas is presumably free from disease germs. Whaling vessels cruising in these seas are in the habit of renewing their supply of fresh water about the beginning of July from fresh water ponds or lakelets, formed on the ice floes by the melting of the snows of the previous winter under the steady rays of the long summer sun. This water is used as fresh water for all purposes on board until the return home, two or three months later; and no suspicion has ever attached to it. Large fleets sail from North Britain yearly on these whaling expeditions, and as the vessels are strongly manned—fifty to eighty of a crew—the case seems clear in favour of the purity of this water. But, however this may be, certain it is that the snow-fall on the backbone of the American Continent is impregnated with deleterious vegetable matters.

In discussing questions of malarial poisoning the profession generally holds in view a pernicious exhalation pervading the air, and affecting the system through cutaneous and pulmonary absorption. Yet by many authorities the possible entry of the poison through the medium of drinking water is adverted to. Thus, Professor Maclean informs[1] us that: "It is a common belief in India that water is capable of absorbing malaria,

[1] Reynold's System of Medicine, vol. i. p. 59.

and that periodic fevers, dysentery, and even cholera, are produced by drinking water so charged." Dr. E. A. Parkes refers to the point at greater length. "In modern times," he says,[1] "the opinion of Lancisi, that the air of marshes is the sole cause of intermittents, has been so generally adopted, that the possibility of the introduction of the cause by means of water as well as air was overlooked. Still it has been a very general belief among the inhabitants of marshy countries, that the water could produce fever." And he then quotes several instances in which paludal fevers were attributed to malarious waters.

Now, although, according to such teachers as make reference to the topic, rapidity of development and fatality of issue are acknowledged characteristics of malarial disease when introduced by water, it must not be forgotten that these impressions are derived from experiences in "notoriously unhealthy" districts, where the water, we may suppose from its malarious surroundings, is strongly charged with the poison. What might be the effect of the continued ingestion of a weakly tainted water does not appear unless the cases above described as mountain fever are allowed to be malarial disease from water impregnation.

Exposure to aeriform malaria produces effects proportioned to the dilution or concentration of the toxic principle. Hence we may expect analogous results from the watery solution. Diluted exhalations yield us dumb agues and mild intermittents, while the concentrated poison of the Indian jungle prostrates the system with a malignant remittent. So the weak solution may be credited with the disordered condition which has been indicated as the first stage of mountain fever; a continuance of ingestion, or an increase of poisonous qualities may develop the other stages; while the strongly charged water of an unhealthy district may at once strike the patient down with a pernicious fever.

In view, then, of the above-recorded experiments on snow and river water, and of the ideas connected therewith, may we not feel warranted in claiming that mountain fever is not dependent upon any peculiar mountain miasm, but is a malarial remittent with adynamic tendencies—and that some plausibility attaches to the theory of its origin in malaria introduced into the water supply by the winter snow-falls? This theory explains such differences in the natural history of the mountain fever poison as would lead one, at first sight, to imagine a peculiar influence with characters specifically distinct from those of malaria; the latter lying low in swampy valleys, amid a luxuriant vegetation, most pernicious in hot climates, and in seasons of the year which correspond with the drying up of the annual floods, and producing in the diluted exhalations of more temperate climes a form of fever intermittent in tendencies; the former prevailing in upland regions, amid a meagre vegetation, and in a climate which from altitude assimilates to the Arctic—its prevalence corresponding not

[1] Manual of Practical Hygiene, p. 55.

with the drying up of the floods, but with their rise and progress, and producing in the system a remittent instead of the intermittent febrile form.

This theory is based upon the following considerations:—

1st. The malarial character of the disease as testified to by the majority of observers, by the influence of quinia, and by the differences in history, symptoms, and habitat between it and enteric fever.

2d. The absence of indigenous malaria in the mountain country, as shown by the reports of certain medical officers who have adverted to this point; although the presence of intermittents would be no argument against the theory, but would rather point to the existence of local sources of exhalation.

3d. The presence of vegetable organic matter, and the probable presence of malaria in all the rain and snow showers, but especially in the heavy large-flaked falls which constitute the mass of the winter's snow. The *probable presence* of malaria in the snow appears to be a weak link in the chain of argument, but it must not be forgotten in estimating its weakness that the *probable presence* is all that can be allowed in marsh air, the *actual presence* being insusceptible of demonstration.

4th. The presence of the same vegetable contamination in the water of the running streams as proved by experiment.

5th. The correspondence in time between the melting of the snow and the endemic occurrence of the mountain remittent—May, June, and July being, as already stated, the months of visitation at Fort Bridger. Now as this post is 7000 feet above the sea level, and, moreover, situated on the northern slope of the mountains, its springs are late. The thaw does not begin until toward the end of April, and the waters of the creek continue turbid and laden with the organic impregnation of the snow until July. Trout fishing, in fact, begins about the middle of that month, and no sport of this character can be entered upon until the waters have fallen and become perfectly free from turbidity.

6th. The correspondence in time between the autumnal rain-caused rise in the stream, and the increased prevalence of the fever as shown by the Fort Bridger record for November.

7th. The correspondence in time between the freedom of the face of the country from snow—a few white patches only being visible on the highest peaks of the range—and the exemption of the post from febrile attacks. For the records show no case of mountain fever during the months of September and October, and these are the months when the stream is at its lowest and purest, so far as it is a question of organic contamination.

8th. The sporadic appearance of mountain fever during seasons when the evolution of malaria from mountain marshes and river valleys is rendered unlikely by the low temperature, as in the cases of hunters, miners, and cattle herders, and, on a larger scale, in the cases above mentioned as having occurred in the Fourteenth Infantry—corresponding with snow-

water drinking in every case investigated. The Fourteenth Infantry during the early portion of their campaign were several times obliged to camp near bad water—melted snow collected in natural tanks—and during the latter part the water supply for all culinary and camp uses had frequently to be derived from the snow covering of the earth. Here the increased prevalence of mountain fever at Fort Bridger during the month of January may be referred to, and attributed to accidental circumstances of a character similar to that which gave Camp Douglas a series of cases in January, 1877.

9th. The fact that mountain fever is not so prevalent now, nor so fatal as in the so-called "early days," corresponding with improvement in the mode of trans-continental travel to the settlements, and improved water supply at them. One cause of the lessened fatality is, of course, to be found in the extended recognition of the power of quinia over the progress of the disease, but the diminished prevalence must be referred to the water supply. The emigrant is now whisked by rail in a few days from the Atlantic coast to the Rocky Mountains, and we never find that his arrival is signalized by prostration from the mountain fever.[1] In earlier times, however, when the trip implied months of weary marching, and a water supply contaminated by melting snow, the disease was, as recorded at the beginning of this paper, both common and deadly. So, in the settlements, well-digging, by furnishing a purer supply than the running stream, the beaver dam, or stagnant pond, has been followed by diminished prevalence.

In this connection I cannot refrain from quoting a suggestion by the late Professor Parkes:—[2]

"Is it not possible," he says, "that the great decline of agues in England is partly due to a purer drinking water being now used? Formerly, there can be little doubt, when there was no organized supply, and much fewer wells existed, the people must have taken their supply from surface collections and ditches, as they do now, or did till lately, at Sheerness."

10th. The appearance of the fever in all its original characters among hunters, miners, prospectors, surveyors, herders, soldiers, or scouting expeditions, and all parties who are thrown for their water-supply on the streams, ponds, pools, dams, and natural tanks which were the sources of supply in the days of the pioneer emigrants.

11th. The appearance of the disease modified by medical supervision at such posts as Fort Bridger, where the water supply is drawn from the running stream.

12th. The rarity of its appearance at such posts as Camp Douglas, where the water is kept comparatively free from malarial contamination. At this post—

"The water is taken from Red Butte Creek, a stream that flows through the reservation from Red Butte Cañon, which is a cut in the mountains, situated to

[1] Dr. Benedict, Salt Lake City.

[2] Manual of Practical Hygiene. Note on p. 56.

the east of the post. About one-third of a mile up the stream the water is turned from its natural channel by a dam 100 feet long and 6 feet high, substantially constructed of stone, timber, and earth, and provided with waste-way and overflow. From the raised pond formed by the dam an open acequia, about 850 feet long, conveys the water to a reservoir of the capacity of 700,000 gallons, which is located on a natural slope above the post and a thousand feet distant from the line of officers' quarters. The reservoir is formed by excavating the side of the foot-hill of the mountains, and the earth removed therefrom, being placed along the lower side and ends, makes a firm embankment 30 feet wide at the bottom, 6 feet wide at the top, and 8½ feet high. The depth of the water in the basin will average 6½ feet. A waste-way and flush-gate constructed of wood are provided, so that the contents of the basin can be run out in a few minutes when desired. The level of the water in the basin is high enough to give a head of 90 feet at the officers' quarters."

This water is distributed by five inch mains, four inch laterals, and three-quarter inch service pipes.

"During the months of February, March, and April the water in the creek is usually very muddy. When that is the case it is desirable that very little water be allowed to run into the reservoir, as the sediment that will accumulate from a large stream will very soon fill the basin. When there is danger of the supply in the reservoir falling short, and water *must* be let in, it will be well for the man in charge to observe the character of the water, and select days for refilling when the stream is less turbid than usual. In the early morning the water will be found more free from earthy impurities than in the afternoon."

The quotations are from the report of Captain Geo. W. Davis, Fourteenth Infantry, who superintended the construction of the post. His suggestions are born of a thorough knowledge of his subject. Yet, had his object been the preservation of the water supply, not simply from suspended matters, but from malarial contamination, he could not have laid down better rules for the guidance of "the man in charge." The result of this is that the reservoir water is always freer from organic impregnation than that of the creek. A comparison, made March 26, 1877, when the stream was by no means turbid, showed the running water to contain .22, while the post supply gave only .16 part per million.

The freedom of the post from malarial remittents can thus be placed in juxtaposition with an improved water supply; it is to be regretted, however, that the health of the garrison anterior to the construction of the reservoir cannot be brought into the question under discussion. Surgeon E. P. Vollum, U. S. A., reports, as above quoted, that the last epidemic of mountain fever occurred in 1871–72. His official records for those years show many quotidian and tertian intermittents, which may, however, have been imported, and 16 cases of typhoid fever, 6 of which proved fatal, but no cases of remittent fever. The typhoid cases may, therefore, be regarded as examples of the mountain fever which begins "as an intermittent, passing on to a remittent, then into a typhoid condition." He records also a large amount of sickness, "chiefly of a febrile and catarrhal character," among the families in garrison, but, as he attributed it, to rotting wood, dampness, and want of ventilation and sunshine about the foundations of the quarters, and as these quarters were abandoned in favour

of substantial stone buildings at the time of the construction of the reservoir, the case must be set aside as too complicated for admission into this argument.

Having thus identified mountain fever as a malarial remittent, and referred it for causation and explanation of its peculiarities to the ingestion of malarious water rather than to exposure to malarial exhalations, there opens for our consideration a larger view than is seen on the slopes of the Rocky Mountains. The necessity for a modification of our accepted theory of malarial disease is apparent. Lancisi's doctrines are too exclusive. Water must be recognized as claiming a higher place in the disease-producing category; and the importance of this recognition cannot be overestimated.

Leaving to my professional brethren in civil life the investigation of malarial forms as caused solely by exhalation, that is, coincident with a pure water supply, or as the result of malarious water with, or, if possible, without the concurrence of aeriform malaria,[1] the position shall be further considered in this paper from a military point of view, as bearing on the origin of the fevers which prevail in camps.

Surgeon J. J. Woodward, U. S. A., in his chapter on camp fevers,[2] enumerates as the prevailing fevers of our army, typhoid, malarial remittent, and typho-malarial, inversely in their order of frequency.

As the majority of the surgeons on duty with our army during the war were drawn from practice in civil life, and were familiar alike with typhoid fever and malarial remittents, it is presumable that enteric fever and remittents from malarious exhalations were readily recognized; but, according to Dr. Woodward,[3] in the fall and early winter of 1861, reports began to come in to the Surgeon-General's office from various quarters that a new form of fever was prevailing in our camps. Official attention was first directed to the fact as occurring in the army of the Potomac. A board, consisting of Surgeon A. N. McLaren, U. S. A., Surgeon G. H. Lyman, U. S. Vols., and Assistant Surgeon M. J. Asch, U. S. A., was convened to investigate this fever, and determine "whether it is to be considered an intermittent or bilious remittent fever in its inception, assuming in its course a typhoid tendency or a typhoid fever primarily"—the very point at issue with regard to our Rocky Mountain fever. The result is interest-

[1] When we find Dr. Jerome Cochran of Alabama, as quoted by Dr. Woodward in his Remarks on Typho-malarial Fever before the International Medical Congress, 1876, denying the existence of abdominal typhus in Mobile, and ascribing the various adynamic fevers which there occur solely to malarial influence, there would seem to be matter for investigation in the so-called "typhoid" fevers of malarious districts.

[2] Camp Diseases of the U. S. Army, Phila. 1863.

[3] Typho-malarial fever; Is it a special type of fever? Being remarks introductory to the discussion of the question in the Section of Medicine. International Medical Congress, Phila. 1876.

ing. The board investigated cases in many division hospitals and collected a great deal of valuable information in writing by means of questions addressed to the brigade and regimental medical officers of parts of the army, which its members were unable conveniently to visit. The general tenor of the replies confirmed the opinion formed by the members of the board on the basis of their own personal observation. This opinion was that, while a certain number of cases of ordinary typhoid fever existed in the army, the large majority of the febrile cases were "bilious remittent fevers, which, not having been controlled in the primary stage, have assumed that adynamic type which is present in enteric fever."

Uninfluenced by this decision, Dr. Woodward, from whose interesting pamphlet this account is abridged, formed the opinion that the prevailing fevers of the army of the Potomac were hybrid forms, resulting from the combined influence of malarial poisoning and of the causes of typhoid fever. He believed that individual cases received their character in accordance as the one or the other of these influences preponderated in the individual. Full of these opinions he suggested as a designation for the complex condition in question the name of "typho-malarial fever," and succeeded in having this term added to the list of diseases printed on the blank form for the monthly sick report. He has often regretted that he did not also urge the preparation of a circular letter explaining why this term had been adopted, and calling for special reports with regard to the cases which it was intended to designate. As it was, however, he goes on to say, "The term went upon the sick report without any explanation, or a word of comment. But, even under these circumstances, 23,346 cases were reported as typho-malarial fever during the following year, showing how widely the opinions I (he) had formed were shared by the medical officers of the army."

It is indeed a matter of regret that Dr. Woodward failed to prepare a circular explanatory of the term, as this neglect, as will be seen directly, affords room for questioning his estimate of the result of its unexplained appearance on the report.

Medical history shows clearly that the Hungarian fever was spotted typhus modified by malarial complications. The propagation of pure typhus in non-malarious districts by sufferers from the army fever leaves no room for doubt on this point. And, if typhus with malaria gives the army surgeon a hybrid to treat in countries where typhus prevails, it is ground for admitting that typhoid may also furnish a hybrid where it is the prevalent fever. But to attribute all adynamic forms of fever occurring in camps to the modified action of the *materies morbi* of typhus or typhoid fever seems to be taking a step beyond the authorization of facts.

The Gottingen epidemic began as a remittent, and, after the fever was fairly under way, presented many of the symptoms of ordinary typhoid; in its treatment extract of Peruvian bark often proved highly efficacious.

This, in brief, is the definition usually given in our Rocky Mountain fever. But in the one as in the other there is no evidence of the presence of enteric fever. It may have been present in Gottingen, very probably it was present, but:—"Dysenteric sloughs frequently existed in the colon. Nowhere, however, do I find any description of the bulky tumefaction, ulceration and sloughing of the glands of Peyer which is characteristic of typhoid fever."[1] The dysentery which existed in connection with this Gottingen epidemic bears strongly on the relative causation of this fever and that which I have described as mountain fever.

The Walcheren epidemic began also as a remittent, speedily, however, running into a continued fever of typhoid type with muttering delirium, small rapid pulse, dry black tongue, sordes-covered teeth, fetid odour, and black discharges from the bowels. Here also, at the autopsies the characteristics of dysentery were frequently found in the colon; although occasional lesions of the small intestines would seem to indicate in certain cases the presence of the enteric fever poison.

In our own war we undoubtedly met with uncomplicated typhoid cases, with typhoid poison in malarious subjects—Dr. Woodward's hybrid—and with recognizable remittents; but were there no others? My own experience leads me to affirm their existence; a period of broken health during which the soldier laboured to throw off his bad feelings and stick to duty, followed by a remittent fever becoming adynamic in its course, with dysentery tending to hasten a fatal issue.

I was on duty in the field to the end of the war, and saw many such cases in their inception, few comparatively at their termination, as field division hospitals were usually kept in light marching condition. Depôt hospitals in the rear afforded better opportunities for studying the convalescence and autopsical appearances. Yet, there were times, as during winter quarters, when field hospitals were permitted to retain and treat such sick of their commands as were within the limit of their capacity. Thus an occasional autopsy would take place, showing such strongly marked dysenteric lesions as to give my mind a tendency to accept the theory of the malarial origin of the case under examination rather than that of its typhoid genesis.

But, in a question of such vast extent as the nature of camp fever, the experience of one becomes swamped in that of the many.

The experience of the many during our late war, as referred to in Dr. Woodward's valuable paper, is comprised in the report of the Board to the effect that the fever was essentially a malarial remittent, and in the popularity of the name "typho-malarial" which Dr. Woodward regards as an endorsement of his own views. With regard to the former I have nothing to say, but the latter requires a word of criticism.

[1] Dr. Woodward, op. cit. .

It must be remembered that the word *typhoid* has become a hack in medical literature, expressing on the one hand the special fever of that name, otherwise known as abdominal typhus or enteric fever, and on the other any low or adynamic condition of system occurring in the progress of a disease unconnected with the poison or germs which produce enteric fever. Under ordinary circumstances the meaning of the word can be arrived at from the context, but when we come to the formation of compound words into which the ambiguous *typhoid* enters, its signification may not be so clear. What is its value in *typho-malarial?* Dr. Woodward has no doubt on the point, because he knows the mental value he stamped on the word when coined; but others are at liberty to give it a different valuation; and in fact its originator seems to acknowledge this in regretting that he did not have a circular issued explaining why the term had been adopted.

I am not prepared to say what amount of endorsement Dr. Woodward can draw from the popularity of the term, but this I know that in my service as medical inspector of the Second Army Corps, I have often stood by the bed of a fever case and spoken of it to the surgeon in charge as typho-malarial fever when the symptoms pointed to a low form of malarial remittent; while when enteric poison was manifest, either in the history of the case, or by the presence of the eruption, typhoid stools or iliac tenderness, all ambiguity was thrown aside and the patient spoken of as affected with typhoid. Malarial complication *might* be present, but it was recognized as a complication, the typhoid element being considered as the disease *par excellence*, the disease which caused death; just as after a battle a gunshot wound of the chest would be regarded as the *primum mobile* of death, although the injured man might be suffering from the effects of malarial poison, and those effects might have contributed in no small degree to the fatal issue by impairing the powers of the constitution.

Nor can it be supposed that I was the only medical officer who made use of the term "typho-malarial" for the adynamic cases which did not present one or other of the accredited symptoms of abdominal typhus. Had Dr. Woodward, instead of the item "*typho-malarial*," placed two terms on the report, one "*adynamic remittent*" for the malarial cases, and the other "*entero-miasmatic*," as suggested by Dr. Geo. B. Wood, for the hybrid form, it is doubtful if the army would have furnished as many cases of the latter as it did of typho-malarial fever. In fact I can only see in the large number of cases thus reported the eagerness with which army medical officers avoided a commitment to the theory of a typhoid fever in a malarious constitution or a malarial fever assuming an adynamic type; while at the same time, on account of the ambiguity of the term, theorists on both sides of the question recorded their cases as typho-malarial, and not those alone who concurred with Dr. Woodward in his pathology of typho-malarial disease.

I am strong in my adherence to the opinion of the Board that the army fevers were remittents "which not having been controlled in their primary stage have assumed that adynamic type which is present in enteric fever."—and my study of mountain fever has tended to confirm me in this belief.

Any one who has seen both diseases cannot but be struck by the resemblance between mountain fever and the camp fevers of our civil war. Some medical officers indeed speak of the mountain remittent as typho-malarial fever, the same term by which, no doubt, they recorded fever cases among the troops from 1861 to 1865. Now, in the history of the miners, prospectors, surveyors, old time emigrants and recent scouting expeditions, antecedent to their prostration by mountain fever, what have we in common with the troops who were the subjects of camp fever during our great war? Exposures to climatic influences, over-fatigue, want of sleep, anxieties, insufficient and badly cooked food, and impure water. Again, what have we in common between the same troops and the garrison of Fort Bridger previous to the appearance of its epidemic? No exposures, no fatigues, no want of food—nothing but the impure water. Of course on account of the vast concourse of men collected in our war-camps, typhoid fever prevailed and spread from case to case, complicated in a majority of instances by the febrile action induced by exposure to malarial exhalations and probably by the ingestion of malaria in their water supply. But in the mountain fever we have assuredly no specific typhoid or enteric element; we have simply a malarial remittent which if uncontrolled in its earlier stages assumes the adynamic type which is present in enteric fever.

If then in mountain fever we have an adynamic remittent uncomplicated with the specific poison of typhoid fever, if also this mountain fever be considered traced to its origin in the vegetable contamination of the drinking water, and if we are at liberty to assume that a certain proportion of our camp fevers were typho-malarial only in the sense of the supervention of adynamia, it is pertinent to ask may not this certain proportion of our camp fevers originate in a contamination of the water supply by malaria, since impurity of water is the only circumstance common to the previous history of the subjects of both fevers?

Dr. Woodward, in insisting on the enteric element in all camp fevers, divides them into two great classes, both comprehended under the title typho-malarial.

1st. Those in which the malarial element predominates over the typhoid; and

2d. Those in which the typhoid element is most prominent.

But in view of what has been written above concerning the absence of the enteric element or specific poison of abdominal typhus in the cases which have been referred to the action of malarious waters, it is suggested

that they are included in, and probably constitute the bulk of, Dr. Woodward's first class. While as to his second, the typhoid (specific) element being prominent, and the disease therefore easily recognized as a well-known specific fever, although occurring in a system more or less poisoned with malaria, there seems as little necessity for a special term to indicate its existence thus complicated, as for the adoption of *scarlatino-malarial*, or *variolo-malarial*, for an outbreak of scarlet fever, or varioloid among the ague-smitten children of a malarious district. Or, granting the necessity for a composite term, why not include the other factor which so often complicated these typhoid cases. If specific typhoid occurring in a malarious system requires the adoption of typho-malarial, typho-malarial fever in a scorbutic is equally entitled to recognition.

Our camp fever would, therefore be classified as follows:—

1st. Malarial fever; the result of cutaneous and pulmonary absorption of malarial exhalations.

2d. Aqua-malarial fever; the adynamic remittents caused by the ingestion of malarious waters.

3d. Typhoid fever; originating in the specific causes of abdominal typhus, and occurring either uncomplicated or complicated in its symptoms and progress by exposure of the subject to malarial exhalations, or deterioration of his constitution by the use of malarious waters.

This is simply a suggestion, but it is pregnant with important results in army sanitation and in the preventive medicine of civil life. It may be impossible to guard the system against malaria (aeriform) either in civil settlements or among troops in the field. A pure water supply, however, is certainly within reach of the former; while a recognition of the possibility of a proportion of our camp fevers finding origin in malarious water would be a step towards preventing their occurrence. The conditions of field service, it must be admitted, imply many difficulties in the way of a pure water supply. There is no time to search for it. That which lies nearest to the camp must be used, and if it responds satisfactorily to the rough tests of the senses, it is used without suspicion. But if we clearly realize the dangers which may lurk in such water, we are forearmed. The reactions of malarious water will be investigated, and the study will evolve processes for the destruction of its contained morbific agent; so that, in progress of time we may be able to say of the *aqua-malarial* section of our camp fevers what we now say of our *aqua-malarial* mountain remittents:—"They were once more common and more deadly than they are now."

CAMP DOUGLAS, UTAH, April 9th, 1877.